attribute of being present in every conscious state, feeling will be on the same footing with cognition which, too, is never absent from conscious activity. That feeling is usually (not always) less prominent as a conscious element than cognition is easily understood if feeling is of a marginal character; it would thus resemble marginal visual sensations which are neglected by us habitually a good part of the time.

2. Another firm foundation for the theory of the coördinate character of feeling and cognition is the fact that of the conscious qualities only pleasure and displeasure may become detached from the situation in which they arise and cling in succeeding conscious states to qualities of *any* of the senses. In the field of cognition only one sense has a slightly similar characteristic, and it differs in two ways from feeling: (*a*) The visual after-image may become attached to a succeeding light-sensation, but here the qualities are within a single sense. (*b*) The persistence of the visual after-image is marked either by a fusion which abolishes the quality that fuses (as when a red after-image fuses with a blue sensation to form purple), or by an entire absence of fusion, in which case the after-image persist as an independent entity (as when the image floats before the eyes and gets in the way of present vision). Now when pleasure or displeasure continues over from a preceding to a succeeding conscious state, it is not fused with another quality in such a manner as to lose its character of pleasure or displeasure, but, remaining what it was, it colors affectively the new state. Very interesting results as to the behavior of the feeling-elements of moods are reached in Dr. Wohlgemuth's experiments.[8]

The case for feeling as a coördinate aspect of conscious activity, therefore, rests partly on the universal presence of one or both of the affective qualities in all conscious states, partly on a certain independence of cognition manifested by feeling in the production of moods. These facts are more significant than the very questionable arguments adduced from attention and localizability.

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THE BASIC ASSUMPTION OF EXPERIMENTAL SCIENCE

IT is often supposed that experimental science inevitably must assume the existence of an external world which, to a certain but very important extent, is not subject to the control of the more or less passive observer. Moreover, it is supposed regardless of what may be the case for such pure or abstract non-experimental sciences

[8] *Op. cit.*, pp. 243–244.

as, let us say, non-Euclidean geometry, the strictly experimental sciences, such as chemistry and physics, could not exist, let alone be understood, unless it is assumed that there is an outer reality whose laws, at least, are not subject to our control and about which we can learn most directly, if not exclusively, by means of the experimental method.

Now it is, of course, a fact that the scientific, and indeed the most anti-realistic metaphysician, does live and act, no matter what his belief may be, as if there were an outer independent order of fact and law to which he must conform. But it must be pointed out that in spite of the practical and, perhaps, if some metaphysicians are correct, metaphysical necessity of this belief, as far as the purposes of science go, be that science experimental or abstract, this assumption that there is an outer independent order of reality is but an unnecessary and even unscientific over-belief. The procedure of either the experimental or the abstract sciences is every bit as understandable, and indeed gains somewhat in metaphysical economy, if stated in a form which does not assume anything as superfluous as the existence of an order of reality back of the percept-concept experience which is the immediate datum of the scientist. All that is really necessary for the scientist to assume to be able to give an intelligent account of his procedure is, (*a*) the percept-concept nexus that forms the prime experience of the individual scientist, (*b*) that there exist certain necessary relations between parts of the percept field, and (*c*) that there is a certain amount of ignorance as to what these necessary relations are. In these terms the problem of the experimental scientist is merely the problem of finding out, by trying, if, when he has a certain selected set of percept data, that is, when he has present to perception the conditions of the desired experiment, he has also present to consciousness the added percept datum expected if his theory as to the relationship between various parts of the percept field is correct.

Thus to the scientist it is immaterial whether he assumes the existence of an external world or not, but (provided only he realizes the theoretically unnecessary and extraneous non-scientific assumptions involved) of course it is highly convenient, if only for the purposes of exposition, to state his results in a form which anyone can understand, and that means in non-subjective terms.

If the existence of an independent external world is not assumed by experimental science, what is involved? It seems to me that Mill was on the right track when he emphasized the permanency of certain factors in experience. This permanency, however, does not lie in the percept data themselves, as Mill seems to have supposed. The percept material itself, the "things" or "substances" which

we are accustomed to suppose lie back of it, are, as was pointed out long ago by Plato, notoriously unstable. Not only do two bits of sense data seem, that is are, constantly changing, never having twice the same inter-relation, but even the "things" of which they are supposed to concern themselves, that is to say the percept data with as much of variance abstracted as is known or suspected to be due to the individual and not inherent in the permanent factors of the percept field, are constantly changing. The constant things of physics, the mol of gold, the standard meter stick, are in fact constantly varying in position, temperature, electrical charge, and even in mass and dimensions. The significant thing about them which is constant, the only thing that the scientist need assume constant, is not the constancy of substance, but the constant truth of certain conditional propositions about them, namely, that under certain standard conditions of temperature, handling, pressure, *etc.*, this given nexus of concept-percept experience, which we call the mol of gold or standard meter, will have the mass of about 197.2 grams, or the length of one meter. And it is to be noted that even here we are not really asserting that a given "thing," the mol of gold or the standard meter, has fixed attributes, nor even that a given sample "matter" or whatever the substance of things is supposed to be has certain properties. What concerns science in such shorthand expressions as "gold is yellow" is not that gold is a simple entity with the invariant property of yellowness—though we are not denying that metaphysically speaking that may be a fact. What does concern science is the permanent truth of a certain conditional statement not attributing attributes to a substance, but asserting the existence of a fixed relation holding between certain attributes and certain other attributes. That "gold is yellow" means to the scientist that under certain limited (standard) conditions there is a fixed relation between selective reflection for yellow light and the atom number (79); or, to put the same fact in a form which avoids even more clearly the assumption of the existence of an external "thing," what is meant by saying "gold is yellow" is nothing more nor less than saying that in a certain limited set of percepts (the limitation being that other percepts of the set must be standard) the percept "yellow color" is inevitably to be associated with the percept nexus "atom number 79." From this it follows that the real fixed entity of the scientist is not some bit of substance, but a proposition, the fixity of which is of the same nature as the truth of any proposition, whatever that is; and though it may be metaphysically necessary to assume some ultimate fundamental substance or fixed stratum of being, say matter or force, on which to tack "properties," science does not need to assume anything so meta-

physical. All it must assume is not some primary substance to which certain properties appertain, but only that if we have certain percepts certain other percepts are invariably present also. The question of ultimate substance it leaves to metaphysics.

In a like manner science leaves to metaphysics the question of the locus of the necessity or compelling power which it must assume correlates under standard conditions "yellow color" with "atom number 79," or rather correlates the percept "yellow" with other percepts associated with atom number, such as relative position of a certain fluorescent line on an X-ray screen. If there is to be any science at all, there must of necessity be some such correlation, but whether the reason for this correlation lies in some law of our percept mechanism, as it may very well, or whether it lies in some logical necessity inherent, say, in the definition of atom number, which logically requires that atom number 79 be associated with a band of selective absorption such that under standard conditions yellow light is selectively reflected, or whether this necessity is physical, it being the physical nature of gold, only to be determined experimentally, to so reflect—all these questions are outside the scope of science. To repeat, the raw question of experimental science is exclusively, "Is such and such a relationship between experienced data invariant or not?"; not, "Does a 'thing' have such and such properties?" or "Is such and such a relation a law of the external world?"

However, these raw questions of fact are not all there is to science, unless, indeed, we consider a purely descriptive account of factual relations science. Science *per se* attempts more than to describe, and though we may allow purely descriptive material, from which nothing is generalized or concluded, a tentative place in an incompletely developed science, a science really becomes scientific when its facts are so related that you can pass from one to the other by a determined route. It must be such that the connection of one fact with another is itself an integral part of that science. That is to say, a body of fact to be scientific must form a system, *i.e.*, an aggregate of fact, the specification of the relationship between which is a part of the specification of the aggregate.

But this is not all. Not only must we in science fix the connections between facts, but, if we are to pass from one body of facts to another by some process of proof or explanation which is sufficient to completely prove or explain the remaining facts, we must admit that science forms a system of the particular kind recently called logical,[1] it being the property of such systems that among the entities of which they are composed (in this case the propositions which con-

[1] This JOURNAL, Vol. XVI, p. 518 (1919).

stitute the science) there are certain ones (the more general laws or postulates) which, once they are given, determine completely some if not all of the remainder (the "proven" or "explained" less general theorems). Thus when once the general laws of science are given it is impossible that the theorems be otherwise. If this were not so and each fact were independent of the other, or if its relations to other facts were not of such a nature, rigorous proof would be impossible, and one law could not be said to be the consequence of another. Thus if we are to have science at all, we must admit some sort of necessity connecting one body of fact unambiguously with another. From the postulates of Euclid it must follow that the sum of the squares on the sides of a right-angled triangle must be equal to the square on the hypotenuse and not equal to some different quantity, for, to put it most briefly, the very essence of science is that of a logical system.

But though this must be admitted, I think, for science in general, and though it forms apparently the basic assumption on which science in general is built, for experimental sciences this assumption takes a particular form which has important consequences.

In the pure non-experimental sciences, if any there be, say in such a science as non-Euclidean geometry where no one would think of appealing to experiment to see if the sum of the angles of a triangle are greater or less than two right angles, and in which no test for truth value is pertinent save the mere fact that the propositions of the science form a logical system, *i.e.*, that theorems can be proven once postulates are given, sciences in which the question of the truth of the postulates themselves is quite outside the scope of the science, it is quite conceivable that this requirement that theorems be proven may be merely a physiological condition the brain structure imposes on thinking, or perhaps merely a convention of the scientist, a rule he has arbitrarily laid down to govern the game of science-making, and that the data of science itself, be they outside real triangles or percept-nexus triangles, are in fact entirely independent entities which really stand in no such determinate relationship as that supposed by the scientist when he proves one from the other. Perhaps it may be supposed the real data of science just are, and that that is all there is to it, and that any dependence of one on the other is a fiction imposed upon them for the convenience or perhaps even by the necessity of the human understanding.

But though such an assumption of the absolute independence of the basic data can, as has been said, be made for non-experimental sciences, that is, for sciences which depend only on self-consistency or other logical tests for their truth value, such an assumption can not be made for the experimental sciences. Here it must be assumed

that the data of science, quite independent of any conscious process of proof, are in themselves related in the way that is assumed in the deduction of one set of laws from another. Here it must be assumed, if experiment is to be used to prove or disprove theory, that in the outer world, or better in the percept-nexus which we are engaged in studying, there actually does exist a set of invariable relationships between facts, so that one fact and not some contrary fact does in reality follow from a given set of general laws, thus quite regardless of convention or any conscious process on our part. For it is to be noted that while in the purely non-experimental sciences, we have, or should have, confidence in our theorems only in so far as we are conscious that the mental or mechanical processes by which we passed from postulate to theorem were correctly carried out, in the experimental sciences, though we may think our reasoning in passing from one set of laws to another was quite correct, we almost always appeal where possible to an experimental verification of our reasoning; for we assume, and this is the basic assumption of experimental science, that, in reality and quite independent of any mistakes we may make in reasoning, the data of science, whatever they are, are in fact so organized that the very existence of one set of laws is inexorably connected with the existence of just those other laws which, in a properly formulated science, can be proven once the first set is given.

It is just this assumption which allows us to use the results of experiment as a check either of our reasoning in passing from one supposed law to another, or, if we have no reason to doubt our reasoning, as a check as to the possible truth of the supposed law we started from. Thus, suppose we had confidence in the first and second laws of thermodynamics, and derived from them by what we supposed to be rigorous processes of proof the law that for dilute solutions of non-dissociated substances the change of the freezing point with the concentration is equal to 1.99 times the square of the absolute temperature divided by the heat of fusion of the solvent. Suppose we try the experiment and find this is not so. There are then several things which may be wrong. (1) Our experimental conditions may not have been as we thought they were, our thermometer may be inaccurate, equilibrium may not have been established, *etc., etc.*, but all these can easily be checked by processes involving no dubious assumptions. (2) Our assumption that our solution is dilute and non-dissociated may be wrong. If it were, certain consequences would follow and these in turn may be checked up by the same sort of processes we are describing. (3) Our proof may be incorrect. (4) Our assumption of the so-called laws of thermodynamics may be wrong. And (5) it may be that all these processes and as-

sumptions are correct, but that we are not justified, just because by a purely mental or at least human process we derived this law from other laws which are so, in fact, in assuming that we have any right to expect the theorem "proven" to be experimentally verified. Now the point to be noticed is that though we may doubt any or all of the first four of these assumptions we never doubt for a moment the fifth; and we would be willing to give up instantly even the laws of thermodynamics themselves, together with all of the consequences which follow from them, once we had convinced ourselves, perhaps even on the basis of a single very accurate measurement and single careful calibration of conditions and instruments, that experiment did not jibe with expectation, this even though the consequence which was tested was many times further remote from the postulates than the one we have just chosen. Never once would we give up the assumption that the data themselves were necessarily interconnected into a logical system, for to do this would be to give up the very possibility of an experimental test of assumptions and reasoning.

This must be noted as perhaps the most important point of this whole discussion for metaphysics, regardless of whether we regard the data of science as things outside, or as percepts, or even concepts. The mere fact of experimental science requires the outer world, if such a thing is to be assumed, to be a logical system in the exact sense stated, or, if we do not assume an outer world, it requires that the raw data of science, be they percepts, pure properties, partial realities, spirit, or what not, be likewise organized into a logical system, the laws of which can be tested by experiment. If the raw data of science are so organized, it apparently is of little effect on the methods and possibility of science whether you assume them composed of spirit, matter, force or what not, or indeed whether you assume that they dwell in an external world, in your mind, in some "Absolute," or nowhere at all.

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REVIEWS AND ABSTRACTS OF LITERATURE

The Psychology of Functional Neuroses. H. L. HOLLINGWORTH. New York: D. Appleton & Company. 1920. Pp. 259.

Professor Hollingworth in his book applies himself to two main projects. The first is represented by an attempt to give in psychological terms the type of reaction presented in psychoneurotic conditions. To do this he reverts to the conception of redintegration which Hamilton first used to indicate the tendency of a complex idea